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Do Healthcare Financing Privatisations Curb Total Healthcare Expenditures? Evidence from OECD Countries

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Abstract

Healthcare privatisations have been advocated as a cure to the increasing healthcare expenditures in advanced economies. Nevertheless, it has not been established whether such policy measures actually curb aggregate healthcare expenditures. This paper quantitatively analyses this question. We use a coherent way to identify *de facto* healthcare financing privatisations across countries over time, i.e. policy induced statistically significant shifts in the public share of healthcare expenditures. Propensity score matching is used to evaluate the effects of privatisations. In other words, an appropriate counterfactual is found to assess what would have happened had a certain privatisation not taken place. The results from 21 OECD countries show that healthcare financing privatisations lead to cost savings in total healthcare expenditures. The estimated average cost saving is of the magnitude 0.75 percentage points of GDP per year, over a 5-year period after the privatisation. The results are robust to various sensitivity tests.

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1. Introduction

Healthcare reforms, including Healthcare Financing (HCF) privatisations have long been proposed as a tool to curb the rising cost of healthcare in advanced countries (OECD 1987, OECD 1992, Oxley & MacFarlan 1995). Several studies have tried to disentangle the consequences of healthcare privatisations, both in terms of quality, equality and costs (Colombo & Tapay 2004, Saltman & Figueras 1998, Tuohy et al. 2004). One common characteristic of these studies is reliance on case study evidence, which is largely descriptive and does not investigate causal relations in a rigorous way.[1] This research cannot establish whether privatisations deliver efficiency increases. In fact, Stabile & Thomson (2014) argue that specific incentive structures matter rather than whether HCF is public or private. Our analysis, however, shows that HCF privatisations are important for Healthcare Expenditures (HCE).

The OECD (1992) argues that HCF privatisations work by improving macro-economic efficiency. If healthcare is largely publicly financed, consumers are not cost conscious. They have incentives to demand more than the optimum level of health care. Similarly, providers have incentives to deliver this. Consequently if payment for healthcare shifts from public to private 'pockets', all else being equal, demand will decline to more optimal levels and total HCE will decrease relative to the status quo. The OECD does not provide solid empirical evidence for this claim, but base the claim on case studies of 7 OECD countries.

However, the privatisation literature suggests that privatisations often happen as a consequence of 'special interest politics' instead of efficiency reasons. For example, right wing market-oriented governments privatise HCF to reduce the re-distributional effects of a public tax financed system. If this rationale is behind privatisations rather than an economic welfare analysis, it is unclear whether privatisations deliver efficiency increases, see Cavaliere & Scabrosetti (2008).

By now a large literature on the determinants of HCE exists. This literature tries to explain why HCE have increased so much in the post-war era. Most of these studies have used a 'determinants approach' where HCE is regressed on variables thought to affect it. The econometric approach suffers from simultaneity and spurious regression relationships (Hansen & King 1996, Amiri & Ventelou 2012, Huarng & Yu 2015). This literature also comes up with suggestions about variables that can be influenced to reduce costs; see Di Mateo & Di Mateo (1998) for a brief overview. Some of these studies look at whether the public share of total HCE impacts total HCE. Leu (1986) finds that a larger public share is associated with higher total spending on health care. Hitris & Posnett (1992) find no effect of the public share of HCE on total HCE.

Thus, it is unclear whether HCF privatisations deliver total cost savings. We evaluate this question empirically using quantitative methods. Following Wiese (2014) we employ a methodology that allows coherent identification of HCF privatisations across countries over time. The methodology is based on

[1] This approach is often argued to be appropriate due to the large differences in the institutional setup between different countries. These differences seemingly make quantification problematic.

structural break test to identify statistically significant breaks in the share of publicly financed HCE. The breaks are treated as potential reforms for which validation using *de jure* evidence of reforms is needed. The identified privatisations are policy induced and makes a statistically significant economic impact. 26 *de facto* HCF privatisations are identified in 21 OECD countries using this methodology.

We estimate the effect of HCF privatisations on the average change in total HCE as percentage of GDP over 3- and 5-year periods following a privatisation. Propensity Score Matching (PSM) is used to identify a reasonable counterfactual. Common economic and political factors from the 'reform trigger literature' are used to predict the propensity scores. To make the results independent of any specific matching technique various matching techniques are used. Over a 3-year evaluation period we find a negative, but non-robust effect. Over a 5-year evaluation period we find a clear negative and robust effect. The economic effect of HCF privatisations is of the magnitude 0.75 percentage points of GDP average yearly saving over a 5-year period.

Due to data restrictions we cannot assess changes in health quality, coverage and inequality that may result from the analysed privatisations. The cost saving may result from parts of the population having no, or restricted access to healthcare, or from decreases in quality. We cannot assess if the estimated effect comes from efficiency increases, or from less desirable effects.

In section 2 the HCF privatisations are identified and the methodology used is explained. In section 3 the propensity score method is put into context along with the data. Section 4 gives the main results, while section 5 investigates the robustness of the results. Section 6 concludes and discusses the implications of the results.

2. Identifying HCF privatisations

2.1 Structural breaks

We employ the methodology of Wiese (2014) to identify privatisations. For clarity the methodology is explained in detail below.

There exist a broad and narrow group of definitions of privatisations in the literature. The broad definition concerns overall shifts in the boundary between public and private involvement in the economic sphere (e.g. Vickers & Yarrow 1991). The narrow definition concerns shifts in ownership (e.g. Roberts & Saeed 2012). The drawback of the broad definition is that it is problematic to operationalize. The drawback of the narrow definition is that it fails to capture shifts from the public to the private domain when no shift in ownership takes place.[2] This is the case with healthcare-financing privatisations (see appendix table A2). However, we can measure public versus private sector involvement instead of ownership. Furthermore, we need to make sure that shifts in involvement are both policy induced and have a statistically significant impact. Therefore, a *de facto* HCF privatisation

[2] This is for example the case with education, law enforcement and healthcare reforms, both in terms of provision and financing.

is defined as *a statistically significant policy induced shift from public to private sector financing of healthcare services*.

We measure to what extent public and private funds finance healthcare. The historical ratio y_{it} of public HCE relative to total HCE (public + private) in country i at time t can be used to identify privatisations.[3]

Using data provided by the OECD, this ratio is calculated as: $y_{it} = \frac{publicfunds_{it}}{publicfunds_{it} + privatefunds_{it}}$. It can be interpreted as the percentage of public financing of total spending. Hence, we have a measure of public relative to private financing of heath care. Table 1 gives summary statistics for 21 OECD countries in the sample.

Table 1: Descriptive statistics for variables used to identify privatisations

Variable	obs.	mean	st.dev.	min.	max.	source:
Public healthcare expenditure % of GDP	924	5.53	1.75	0.84	9.76	OECD.org
Private healthcare expenditure % of GDP	935	2.08	1.36	0.11	9.03	OECD.org
Total healthcare expenditure % of GDP (private + public)	966	7.59	2.27	1.49	17.05	OECD.org
Public relative to total expenditure, y_{it}	924	0.73	0.13	0.22	0.98	Calculated

All available observations for the 21 OECD countries between 1960-2013 have been used. Belgium is excluded, see table 2 for countries and sample periods. West German data is used prior 1990 for Germany.

Then structural break testing is applied to identify significant shifts in y_{it}. A structural break is the timing at which a fundamental change in the Data Generating Process (DGP) occurs, for example due to an economic reform.[4] However, a structural break can be caused by other factors, such as exogenous shifts in consumer preferences, or relative price movements. Thus, the detected structural breaks need to be validated.

Perhaps the best-known structural break test is the Chow-test. However, the test only appropriate to examine whether a time series contains a single structural break. When using this test the sample is split at a point in time where *a priori* information leads one to expect a break. A F-test is then performed to determine whether subsample parameters are significantly different. For the application at hand, *de jure* evidence gives *a priori* information of several potential structural breaks in each time series (up to 25, see Healthcare Systems in Transition (HSiT) country reports). This means that the time series would have to be split into a large number of subsamples on which the Chow-test should be performed. This is infeasible because the time series not are long enough when there are so many potential breaks. Furthermore, there is often a time lag before a *de jure* reform manifests itself in the data; institutions are rigid (Acemoglu et al. 2006). This means that the division of a time series into subsamples would be arbitrary.

[3] Public healthcare expenditure is defined as: "*health expenditure incurred by public funds. Public funds are state, regional and local Government bodies and social security schemes. Public capital formation on health includes publicly financed investment in health facilities plus capital transfers to the private sector for hospital construction and equipment*" (OECD.org). Private healthcare expenditure is defined as: "*Privately funded part of total health care expenditure. Private sources of funds include out-of-pocket payments (both over-the-counter and cost-sharing), private insurance programs, charities and occupational health care*" (OECD.org).

[4] Seminal examples of structural breaks are: the unification of Germany, and the introduction of a common European monetary policy.

The feasible approach is to start from the economic data and then use *de jure* evidence for validation. Hence, the number and timing of structural breaks are treated as unknown a priori. Bai & Perron (1998, 2003) (B&P hereafter) develop a general method for this purpose. In order to define potential privatisations (and nationalisations) in the context of the B&P-filter, consider a model with m possible structural breaks in an OLS regression framework that takes the form:

$$y_t=\delta_j+u_t \qquad (t=1,...,T\ ,\ \ j=1,\dots,m+1)$$

Where y_t is the dependent variable, in this case the time series of public relative to total healthcare expenditure for each country considered. δ_j is a vector of estimated coefficients (constants) of which there are m+1, i.e. δ_j is the mean at the different segments of the time series y_t. u_t is the error term. The segments generate a stepwise linear route through the times series y_t and give m structural breaks. The idea underlying the B&P-filter is straightforward.[5] It generates the segmented route through the series that yields the lowest Sum of Squared Residuals (SSR) up to a maximum number of breaks. The maximum number of breaks is restricted by a trimming parameter h, which specifies a minimum number of observations that has to occur between consecutive breaks

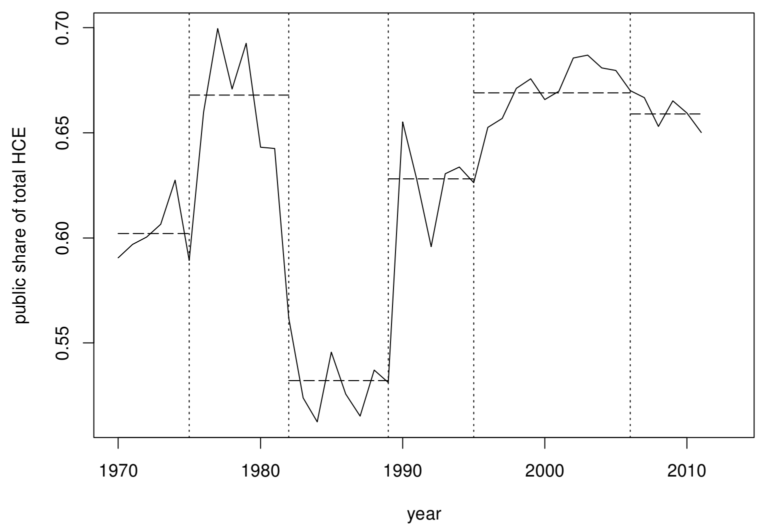

Fig 1. Structural breaks in healthcare financing source: The case of Portugal

In the context of Fig. 1, the segments can be thought of as regimes where y_t fluctuates around the constant mean δ_j. A downward (upward) regime shift is detected as a potential privatisation (nationalisation), for which validation is required. A shift to a new regime is unlikely to happen by chance, dependent on the test-size employed. We employ a 5% significance level. Thus, a regime shift implies that the underlying DGP has been altered generating a structural break.

When applying the B&P-filter 3 general test procedures are possible, see B&P (1998, 2003):

[5] The process underlying the algorithm is also straightforward. First, it searches for all possible sets of breaks up to a maximum number of breaks, restricted by the trimming parameter chosen, and determines for each number of breaks the set that minimises the SSR (Sum of Squared Residuals). Then F-tests determine whether the improved fit produced by allowing an additional break is sufficiently large, compared to what can be expected randomly, on the basis of the asymptotic distribution derived in B&P (1998). After determining the appropriate number of breaks the program extracts the corresponding break dates of the optimal sequential route. The trimming parameter h is expressed either as a fixed number of observations, or a percentage of the number of observations. Autocorrelation, trending time series and non-constant errors are permitted (Bai & Perron 2003).

1. Compares the fit of global *L* breaks with the fit of a model with no breaks, and selects the highest number of breaks that are significant.
2. Starts with a H_0 of no break, and then sequentially test *k* vs. *k*+1 breaks until the test statistics is insignificant.
3. Picks the lowest value of the BIC (Bayesian Information Criteria) to select the appropriate number of breaks.

We chose procedure 1 as our baseline identification method. The reason is that method 2 is too conservative, only 5 potential privatisations are identified using this method, see table A1 in the appendix. This is not enough to estimate a credible average treatment effect. Furthermore, the *de jure* evidence of reforms suggests many more privatisations. Using the Bayesian Information Criterion (BIC) for model selection identifies a somewhat different set of potential privatisations compared to our preferred method, see table A1 in the appendix. B&P (2003) show that information criterion biases the number of identified breaks downwards, but least so if the BIC is used. We find one potential privatisation less using BIC and differences in the identified potential privatisations (country-year pairs) compared to our baseline method, see table A1 in the appendix.

Furthermore, when applying the B&P-filter a choice has to be made concerning the size of the trimming parameter *h*. If the times series do not exhibit autocorrelation or heteroskedaticity any trimming will work regardless of sample size (Bai & Perron 2003). With finite samples that do exhibit autocorrelation and/or heteroskedaticity, like our data series, the trimming needs to be increased. Here a trimming of *h*=0.15 or *h*=0.2 is chosen because it generates the best fit with *de jure* evidence while still being econometrically sound.[6] The trimming parameter implies that no potential privatisation can be identified at the beginning and end of each series. The appropriate observations are excluded in the estimations that follow to avoid identification error. A Heteroskedasticity and Autocorrelation Consistent (HAC) estimate of the variance-covariance matrix is used, see Antoshin et al. (2008).

2.2 Healthcare privatisations

The outcome of running the B&P-filter on the 21 OECD countries can be found in column 3 in table 2. Column 4 shows the potential privatisations that can be validated, see table A2 in the appendix for details. 1960-2013 is selected as sample period. For some countries data is unavailable for the whole period. In that case the longest data period available is used, see column 2 for exact sample periods.

Table 2: Identified privatisations, baseline filter specification

Country	**Sample period**	**Global L breaks vs. none, B&P test statistics 5% significance level**	**Validated**
Australia	1971-2011	1977	1977
Austria	1960-2012	1967	1967

[6] Choosing the trimming to be a percentage of the sample size implies that the number of minimum observations that can occur between breaks becomes lower for shorter samples. Bai & Perron (2003) argue that shorter samples exhibiting autocorrelation and heteroskedasticity calls for larger trimmings in percentage of the sample size. So, we chose *h*=0.2 for the shorter samples, see table A2 in the appendix.

Canada	1970-2012	1986, 1993, 1999	1986, 1993
Denmark	1971-2012	1984, 1990	1984, 1990
Finland	1960-2013	1994	1994
France	1990-2012	2003	2003
Germany	1970-2013	1983, 1998, 2004	1983, 1998, 2004
Greece	1987-2011	1994	1994
Iceland	1960-2013	1993	1993
Ireland	1960-2012	1985, 2006	2006
Italy	1988-2013	1994	1994
Japan	1960-2012	--	--
Netherlands	1972-2002	--	--
New Zealand	1970-2011	1990	1990
Norway	1960-2013	1980, 1988, 1997	1988, 1997
Portugal	1970-2011	1982, 2006	1982, 2006
Spain	1960-2012	1995	1995
Sweden	1970-2012	1985, 1992, 2001	1985, 1992, 2001
Switzerland	1985-2012	--	--
UK	1960-2012	1985, 1997	1985, 1997
USA	1960-2012	--	--
	Total	29	26

Data source for validated privatisations: HSiT (Healthcare Systems in Transition) country reports; see table A2 in the appendix for details.
-- means that the filter does not identify any potential privatisation. Belgium was excluded from the sample because the time series is too short to run the B&P-filter.

It is possible that factors outside control of the policy-maker move the ratio y_t significantly and hence look like a reform when it in fact was not. Therefore, it is checked whether the detected privatisations are likely to result from planned policy. For that purpose the WHO's and European Observatory on Health Systems and Policies "Healthcare Systems in Transition" country report series were employed. These reports are available for each country covering the sample period and have descriptions of *de jure* reforms introduced over time. When a report describes a policy reform that directly or indirectly could have had the objective to either reduce the public share of HCF, increase the private share of HCF, or both, it is taken as evidence of a *de jure* reform. A time lag is present between the *de jure* reforms and their outcomes (Acemoglu et al. 2006). In most cases the length of this lag is one year (see table A2 in the appendix). If more than two years passed between a *de jure* reform and a detected structural break, the reform is not coded as a *de facto* privatisation. See column 4 in table 2 for the outcome of this analysis, and table A2 in the appendix for a description of the related *de jure* reforms. As we are interested in whether healthcare privatisation policies work, only the *de facto* privatisations are used when estimating the effect on total HCE. The non-validated privatisations may result from relative price movements, which may also reduce total healthcare spending. But it is hard to pinpoint whether this is a result of planned policy.

In sum, the analysis reveals that 26 of the 29 detected privatisations can be validated. We are therefore confident that these 26 structural breaks are the result of planned policy, and therefore match the definition of a *de facto* HCF privatisation. Years in which a privatisation is detected and validated are coded as 1, the remaining years as 0. This constitutes the Treatment Identification Variable (*TIV)* used in the estimations that follow.

2.3 Case study

Before we proceed with estimations of the effect of healthcare privatisations on total HCE we end this section with a case study. This is done to highlight the benefits of the presented methodology. Without loss of generality the case of Norway is selected.

The identification of healthcare privations based on the B&P-filter alone identifies a potential privatisation in 1980. The HSiT (Johnsen 2006) report provides an overview of healthcare reforms in Norway through time. The report does not describe any reforms within the two years preceding 1980. Had we relied solely on economic output data to identify the reforms we would have concluded that a privatisation had taken place. However, using *de jure* data to verify the detected reforms prevents such identification errors.

The other possible method that has been argued for in the literature is the use of policy input data alone. However, this approach is at least as problematic as using economic outcome data alone. The HSiT (Johnsen 2006, p.125) report provides an overview of "Major health care reforms and policy measures" from 1984 to 2004. In that period more than 10 reforms occurred that potentially could have had a significant impact on whether HCF would shift from public to private payment. Thus, without application of outcome data we would have made several identification errors, as only 3 breaks are identified in Norway. Additionally, when relying on policy input data alone it is necessary to interpret reform descriptions. What was the primary intention of a given reform, a privatisation or a nationalisation, if any? Often, as supported by the statistical analysis above and the HSiT reports, the objective of the reforms was to improve the efficiency of the healthcare system. Thus, the combination of both economic outcome data and policy input data offers an objective way to identify HCF privatisations.

A risk of the methodology is that that the outcome of a *de jure* reform can be hidden in the data by unrelated economic changes, such as exogenous shifts in consumer preferences or relative price movements. The opposite can also happen, i.e. a policy change has no significant impact on the data, but unrelated economic changes lead us to conclude that it had. Either way, our sequential procedure is less prone to identification error than identification using policy input data or economic outcome data alone.

3. Propensity score matching and determinants of HCF privatisations

3.1 Propensity score matching

Non-parametric estimators have become common to assess the effect of a certain treatment in the absence of experimental data. Not only for analysis in medical science (Rosenbaum & Rubin 1983), but also in various fields of economics such as: Labour Economics (Heckman et al. 1997), Political Economics (Person et al. 2001) and Micro Finance (Imai et al. 2010). Propensity score matching has become a standard tool to assess the effects of treatments like (policy) interventions. Therefore we only briefly review the main idea behind the method, see Rubin (1974, 1977) for more details.

Consider our group of 21 OECD countries of which some in certain years experience a healthcare privatisation. In these years the Treatment Identification Variable *TIV*=1, in all other years *TIV*=0. We are interested in whether the non-random assignment of this treatment has an effect on total HCE. The hypothesis is that it has negative effects (declining HCE); the alternative is that it has no effect. It is well known from the economic reform literature that the effect of reform follows a J-curve pattern of short-run costs, and longer run gains (Hellman 1998). Therefore, the outcome variable is defined as the 'average change in total HCE' over a 3- and 5-year period following a treatment.[7]

Ideally we would like to know what would have happened to a given country had the country not privatised HCF. However, such a counterfactual cannot be obtained in a non-experimental setting where treatments are assigned non-randomly. It is rather likely that the countries that undergo healthcare privatisations are the ones were there is a potential for total cost savings. This implies selection of treatment that will bias any OLS estimate.

PSM offers a way to identify an appropriate counterfactual, and hence a way to reduce selection bias. The method consists of two steps. First a logit (or probit) model is used to estimate probabilities of receiving a treatment, i.e. the propensity scores. Second, different matching techniques are used to match each country-year observation that received a treatment with different country and/or year observations that did not receive a treatment, but is similar on observable characteristics.[8] After this the Average Treatment effect on the Treated (ATT) can be calculated. ATT is the average difference between the outcomes in treated countries and the matched counterfactuals.

The crucial assumption underlying the method is 'conditional independence'. No omitted or unobservable variable influences both the choice of healthcare privatisation and total HCE once we have controlled for variables effecting treatment assignment in the first step. This assumption allows us to replace the unobservable ideal counterfactual with an observation with similar characteristics as the treated observation given the propensity score. After this we can assess whether the assigned treatment has an effect, independent of selection bias.

3.2 Determinants of HCF privatisations

Due to the 'conditional independence assumption' a central part of PSM is selection of an appropriate set of covariates to estimate the propensity scores. We rely on the literature on economic and political factors that trigger economic reforms and privatisations. Specifically, we consider economic, political and demographic factors that are believed to cause total HCE, and therefore also HCF privatisations. Also, we strive for a parsimonious model, see table A3 in the appendix for descriptive statistics of the covariates used for matching.

[7] Since our outcome variable is the average over 3 or 5 observations we drop observations from the control group before and after a treatment in a given country-year observation. For example, for the 5-year average outcomes, we drop the 4 preceding and the 4 following observations from the control group. Otherwise, an observation with *TIV*=1 could be matched with a *TIV=0* observation in which the outcome contains part of the outcome from the *TIV*=1 observation. Neglecting this would lead to biased estimates.

[8] In principle a treatment can be matched with control(s) from the same country. However, this is not a problem if this is the best counterfactual based on observable characteristics. It can be argued that controls from the same country are the best counterfactuals.

Economic crises are perhaps the most common factor thought to trigger economic reforms in general. Rodrik (1996) claims that crises are a sufficient and necessary condition for reform, like smoke following a fire. Drazen (2000) shows theoretically why this may be the case. The empirical evidence is robust; crises trigger reforms (e.g. Drazen & Easterly 2001, Pitlik & Wirth 2003, Wiese 2014). Therefore, several measures that capture different economic crises are included: the growth rate of GDP, the unemployment rate and the severity of government indebtedness.[9] In times of crises both governments and consumers are likely to cut HCE, at the same time privatisations become more likely. The Health Economics literature on the determinants of HCE has consistently found that GDP determines it (De Mateo & De Mateo 1998). Furthermore, the *inflation rate* is included because governments can use their power to issue new money to finance fiscal expenditures, and therefore also rising costs to public health care. At the same time a high inflation rate may signal economic crisis, so the probability of reform increases.

Special interest politics may drive privatisations. In the political economics literature this is referred to as the 'partisan hypothesis' (Hibbs 1977). Political parties promote policies that favour their constituencies. Left-wing governments prefer a public system and right-wing market-oriented governments favour privatisation. Therefore, the Potrafke (2009) index of the ideological orientation of governments, in terms of economic policy, is included. Also, it is well established both theoretically and empirically that a more fractionalised government finds it harder to agree on welfare improving reforms (Alesina & Drazen 1991). Therefore, the measure of political fractionalisation of governments of Beck et al. (2001) is included.

Two variables that capture cost developments that are likely to impact the probability of health care financing privatisations are included. First, the percentage of the population more than 65 years old is included. This is an important factor driving the costs of healthcare because a larger fraction of elderly implies higher costs and declining tax revenues to finance the costs (Di Mateo & Di Mateo 1998, Oxley and McFarlan 1995). Second, we include a variable capturing the medium term trend in healthcare costs directly. The variable is the average of the 5 previous years total HCE as percentage of GDP. The reason to include it is, that if costs are rapidly increasing it calls for strong policy action, such as HCF privatisations. This variable captures both demand and supply driven costs increases in the medium term, such as technological advances that impact costs and hence also privatisation decisions.

Lastly, duration dependence in panel models with a binary dependent variable makes wrong inference likely if not taken into consideration. Maximum likelihood estimators rely on the assumption that the probability of privatisation within countries is independent over time. Beck et al. (1998) show that panel data with a binary dependent variable is identical to grouped duration data, and propose a simple method to correct for temporal dependence. The method is based on the construction of a set of dummy variables counting the length of the spell of no privatisation at every observation, counting from the last

[9] The severity of government indebtedness is captured by the interest rate on long-term government debt. This variable captures the financial markets' judgement of the sustainability of debt. The debt-to-GDP ratio does not capture the sustainability of debt.

year of privatisation. The intuition is that the length of the spell has an impact on the probability that a privatisation will occur. Including these spell dummies has a serious drawback; they take up many degrees of freedom. To mitigate this problem, Beck et al. (1998) propose the construction of three cubic splines that mimic the spell dummies by creating a smoothened function for duration dependence. Additionally, they propose to include a variable that counts the number of previous privatisations, and a variable that counts the length of the spell since the previous privatisation. All three suggestions are included in the estimations (see also Mierau et al. 2007, Wiese 2014). Previous privatisations may impact the probability of further privatisations; at the same time the privatisations are likely to impact total HCE. So, we need to control for duration dependence when estimating the propensity scores.

The identified privatisations are used as dependent variable in a binary dependent variable logistic regression. In this case the observational rule can be explained by an underlying latent variable, namely $y_{it}^{*} > 0$ if we observe a privatisation, i.e. $TIV = 1$. In terms of the underlying latent variable this means that the government only decides to privatise when the (expected) benefit from doing so is positive, if $y_{it}^{*} \leq 0$ we will not observe a privatisation, i.e. $TIV = 0$.

When the logit estimator is applied $y_{it}^{*} = x_{it}'\beta + \varepsilon_{it}$ is interpreted as the government's inclination to privatise. x_{it} is a vector containing our covariates, β is a vector of coefficients to be estimated and ε_{it} is a vector of random errors. The probability of privatisation is: $pr(TIV = 1) \rightarrow pr(y_{it}^{*} > 0) \rightarrow pr(\varepsilon_{it} > -x_{it}'\beta) \rightarrow F(x_{it}'\beta)$ where F is the logistic cumulative distribution function that ensures that the estimated propensity scores are bounded between zero and one.

Running this model on our sample of 21 countries and 565 observations results in an estimate with a pseudo-R^2 of 12.8%. The *debt crisis indicator, unemployment rate, inflation rate* and the *percentage of the population over 65* are significant at the 5% level with the expected signs. The political variables are insignificant. The duration dependence variables are jointly significant at the 5% level; this provides evidence that they should be included in the model (see Beck et al. 1998).[10]

[10] For the alternative BIC filter specification the pseudo-R^2 is 21.6%. The same covariates are significant with the expected signs. See Fig. A1 in the appendix for a common support graph.

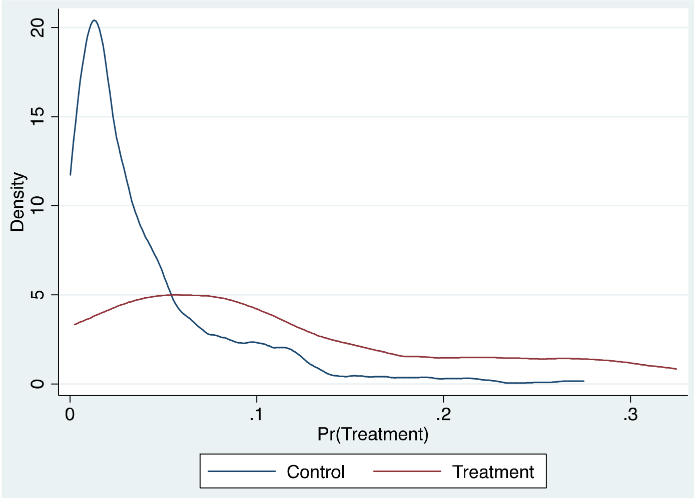

Fig. 2: Common support distribution for the baseline propensity scores

Fig. 2 shows the estimated propensity scores for the treated observations and the control observations for the privatisations identified using the baseline B&P-filter specification. Concerning treatment observations with a propensity score close to 0.3 or above, there is no overlap in the propensity score distributions, i.e. there are no suitable counterfactual(s). These observations are dropped.[11] For the remaining treatment observations there are good counterfactuals, which have a similar propensity of receiving a treatment based on observable characteristics.

Following Aidt and Franck (2015), we apply 5 different matching algorithms with the objective to use methods that are dissimilar.

1: Nearest neighbour matching: for each *TIV*=1 observation it selects the observation from the control group with the propensity score closest to the treated observation. We impose that the same control observation cannot be used multiple times as a match, i.e. matching without replacement.

2: 5 nearest neighbours with replacement. Aside from performing the matching with replacement, it matches the treated observation with the five closest observations from the control group in terms of their propensity score.

3: Nearest Neighbour with replacement, imposing a calliper distance. Using this approach, pairs of treated and control observations are formed such that the difference in propensity scores between matched subjects differs at most by 1% in probability.

4: Kernel bootstrap. It constructs a match for each treated observations using a weighted average over multiple observations in the control group.

5: Kernel bootstrap with trimming=3. In contrast to the fourth approach it imposes common support by trimming 3 percentages of the treatment observations at which the propensity score density of the control observations is the lowest.

[11] The privatisations in Denmark 1984, Finland 1994, Spain 1995 and Sweden 1992 are dropped.

4. Main results

The results indicate that HCF privatisations lead to a significant HCE saving in the medium run. In the short run the results suggest a less strong effect. Table 3 shows the ATT for the baseline identification strategy of HCF privatisations for different matching algorithms over the 3- and 5-year time horizon. 22 of the 26 validated healthcare privatisations are used, while 4 observations with *TIV*=1 are dropped due to the lack of common support (see fig. 2 and footnote 12).

Table 3: Baseline ATT estimates using different matching algorithms

Variables	(1) **Nearest Neighbour without replacement** (t-stat)	(2) **5-Nearest Neighbours with replacement** (t-stat)	(3) **Nearest Neighbour with replacement & calliper=0.01**[1] (t-stat)	(4) **Kernel bootstrap se** (p-value)	(5) **Kernel with trimming=3 bootstrap se**[2] (p-value)
Total HC expenditures as % of GPD - 3 years average	-0.107*	-0.040	-0.107*	-0.064	-0.067
	(-1.83)	(-0.79)	(-1.83)	(0.185)	(0.162)
Total HC expenditures as % of GPD - 5 years average	-0.073**	-0.073***	-0.073**	-0.074**	-0.079***
	(-2.01)	(-2.35)	(-2.01)	(0.013)	(0.009)

Notes: This table contains the ATT using the baseline B&P-filter specification. All estimates are on the common support. For the bootstrap we use 1000 replications. *** p<0.01, ** p<0.05, * p<0.1

[1] Value for maximum distance of controls

[2] imposes common support by dropping # percentage of the treatment observations at which the propensity score density of the control observations is the lowest

As expected, the ATT for the 3-year average health care expenditure as percentage of GDP is less substantial than the 5-year average. The results weakly indicate that healthcare costs decline over the 3-year evaluation period. All average treatment effects are estimated with a negative sign, only matching method 1 and 3 are significant at the 10% level. The remaining effects are insignificant.

Concerning the ATT for the 5-year average HCE as percentage of GDP the results clearly show a substantial cost saving from privatisations. All five matching techniques are significant at the 5% level; techniques 2 and 5 are significant at the 1% level. The effect of a HCF privatisation is estimated consistently across matching techniques. The results suggest an average cost saving over the 5-year period of about 0.75 percentage points of GDP each year. Accumulated this means that approximately 3.75 percentage points of GDP are saved over five years. This is a substantial economic effect. Given our estimate for the 3-year average effect, the effect mainly comes from the 4th and 5th year following the privatisation. This is in line with the literature that evaluates the economic consequences of economic reforms. A central finding in this literature is that the economic effects of reforms follow a J-curve pattern. First there are short-run (adjustment) costs and in the longer run the benefits are realised (Hellman 1998). It would be interesting to evaluate the effect over a longer time horizon. But given our empirical strategy this means that too many observations have to be dropped from the control group. Additionally, over a longer time horizon the probability that other factors impact the outcome variable becomes too large.

5. Robustness analysis

To investigate whether the specification of B&P-filter used to identify potential healthcare privatisations impacts the results we redo the analysis in section 4. That is, we use the BIC to select the optimal number of breaks and the corresponding timing of the breaks. Using this specification we identify 28 potential privatisations of which 25 can be validated. There are also differences concerning the countries and the timing (see table A1 in the appendix for the privatisations identified using this filter specification).

The results are less strong using the alternative filter specification; see table 4. None of the estimated ATT effects concerning the 3-year averages are significant. Concerning the 5-year average effects matching method 1 is significant at the 10% level with the expected sign, and method 3 is marginally insignificant. Nevertheless, all signs are negative and the estimated effects are in line with the baseline results. B&P (1998) shows that the use of information criteria to select the break dates is less precise compared to the asymptotic B&P-test. This may be driving the differences compared to the main results.

Table 4: ATT estimates using different matching algorithms (privatisations identified using BIC for model selection)

Variables	(1) **Nearest Neighbour without replacement** (t-stat)	(2) **5-Nearest Neighbour with replacement** (t-stat)	(3) **Nearest Neighbour with replacement & calliper=0.01**[1] (t-stat)	(4) **Kernel bootstrap se** (p-value)	(5) **Kernel with trimming=3 bootstrap se**[2] (p-value)
Total HC expenditures as % of GPD - 3 years average	-0.048	-0.039	-0.058	-0.018	-0.060
	(-0.87)	(-0.79)	(-1.00)	(0.683)	(0.228)
Total HC expenditures as % of GPD - 5 years average	-0.072*	-0.048	-0.067	-0.031	-0.047
	(-1.80)	(-1.27)	(-1.60)	(0.374)	(0.186)

Notes: This table contains the ATT using the BIC information criteria. All estimates are on the common support. For the bootstrap we use 1000 replications. *** p<0.01, ** p<0.05, * p<0.1

[1] Value for maximum distance of controls

[2] imposes common support by dropping # per cent of the treatment observations at which the propensity score density of the control observations is the lowest

Also, the inclusion of the duration dependence variables may be a cause of concern, as it is unclear whether their inclusion in estimating the propensity scores causes the estimated effects. Therefore the baseline specification in section 4 is redone without their inclusion. Excluding these variables does not cause the main result to vanish, see table 5. On the contrary, the estimated effects are stronger. For the 3-year averages the results of matching method 1, 3, 4 and 5 are significant at the 10% level. For the 5-year averages the results of all but method 2 are significant at the 1% level, method 2 at the 5% level. The estimated 5-year average cost saving is between 0.7 and 1 percentage point of GDP. So the main result is not driven by the inclusion of duration dependence variables.

Table 5: ATT estimates using different matching algorithms (baseline privatisations) excluding splines

Variables	(1) **Nearest**	(2) **5-Nearest Neighbour**	(3) **Nearest**	(4) **Kernel**	(5) **Kernel with**

	Neighbour without replacement	with replacement	Neighbour with replacement & calliper=0.01[1]	bootstrap se	trimming=3 bootstrap se[2]
	(t-stat)	(t-stat)	(t-stat)	(p-value)	(p-value)
Total HC expenditures as % of GPD - 3 years average	-0.104*	-0.076	-0.103*	-0.083*	-0.083*
	(-1.91)	(-1.62)	(-1.81)	0.065	(0.071)
Total HC expenditures as % of GPD - 5 years average	-0.104***	-0.067**	-0.092***	-0.081***	-0.077***
	(-2.94)	(-2.33)	(-2.39)	(0.004)	(-2.76)

Notes: This table contains the ATT using the baseline B&P-filter specification. All estimates are on the common support. For the bootstrap we use 1000 replications. *** p<0.01, ** p<0.05, * p<0.1

[1] Value for maximum distance of controls

[2] imposes common support by dropping # percentage of the treatment observations at which the propensity score density of the control observations is the lowest

Likewise, it is important to establish whether individual countries included in the sample are driving the results. In table 6 we re-estimate the baseline results for the 3-year average effect excluding each individual country one at the time. The main outcome is that the inclusion of Italy and Spain in our main results causes them to be less strong and insignificant. Also, if Sweden or Switzerland is excluded the effects that are significant at the 10% level in the main results become insignificant.

Table 7 does the same for the 5-year averages. The exclusion of Italy causes the estimated ATT to increase. Both table 6 and 7 show that the main results not are driven by the inclusion of any specific country.

Table 6: ATT estimates using different matching algorithms excluding individual countries (three years average)

Variables	Country excluded	(1) Nearest Neighbour without replacement	(2) 5-Nearest Neighbour with replacement	(3) Nearest Neighbour with replacement & calliper=0.01[1]	(4) Kernel bootstrap se	(5) Kernel with trimming=3 bootstrap se[2]
		(t-stat)	(t-stat)	(t-stat)	(p-value)	(p-value)
Total HC expenditures as % of GPD - 3 years average	Australia	-0.086	-0.031	-0.086	-0.061	-0.064
		(-1.43)	(-0.58)	(-1.43)	(0.249)	(0.217)
-	Austria	-0.112*	-0.039	-0.112*	-0.067	-0.069
		(-1.93)	(-0.77)	(-1.93)	(0.170)	(0.154)
-	Canada	-0.107*	-0.040	-0.107*	-0.064	-0.067
		(-1.83)	(-0.79)	(-1.83)	(0.177)	(0.171)
-	Denmark	-0.107*	-0.026	-0.107*	-0.044	-0.048
		(-1.73)	(-0.49)	(-1.73)	(0.386)	(0.321)
-	Finland	-0.116*	-0.038	-0.116*	-0.058	-0.061
		(-1.95)	(-0.71)	(-1.95)	(0.254)	(0.233)
-	France	-0.089*	-0.040	-0.089*	-0.065	-0.068
		(-1.65)	(-0.80)	(-1.65)	(0.170)	(0.172)
-	Germany	-0.111*	-0.044	-0.111*	-0.070	-0.072
		(-1.82)	(-0.83)	(-1.82)	(0.177)	(0.136)
-	Greece	-0.108	-0.023	-0.108	-0.055	-0.059
		(-1.61)	(-0.40)	(-1.61)	(0.325)	(0.286)
-	Iceland	-0.107*	-0.039	-0.107*	-0.064	-0.064
		(-1.83)	(-0.76)	(-1.83)	(0.203)	(0.203)
-	Ireland	-0.041	-0.022	-0.041	-0.062	-0.066
		(-0.68)	(-0.41)	(-0.68)	(0.220)	(0.178)
-	Italy	-0.134***	-0.065	-0.134***	-0.089*	-0.090**
		(-2.36)	(-1.34)	(-2.36)	(0.066)	(0.044)
-	Japan	-0.106*	-0.030	-0.096	-0.051	-0.051

		(-1.77)	(-0.56)	(-1.56)	(0.300)	(0.325)
-	Netherlands	-0.107*	-0.046	-0.107*	-0.066	-0.066
		(-1.83)	(-0.90)	(-1.83)	(0.181)	(0.185)
-	New Zealand	-0.107*	-0.040	-0.107*	-0.064	-0.067
		(-1.83)	(-0.79)	(-1.83)	(0.189)	(0.154)
-	Norway	-0.102*	-0.055	-0.107*	-0.069	-0.069
		(-1.69)	(-1.07)	(-1.78)	(0.168)	(0.156)
-	Portugal	-0.092	-0.065	-0.092	-0.081*	-0.083*
		(-1.57)	(-1.29)	(-1.57)	(0.084)	(0.064)
-	Spain	-0.133**	-0.078	-0.133**	-0.091*	-0.092*
		(-2.21)	(-1.58)	(-2.21)	(0.055)	(0.055)
-	Sweden	-0.081	-0.018	-0.081	-0.042	-0.042
		(-1.30)	(-0.33)	(-1.30)	(0.431)	(0.410)
-	Switzerland	-0.081	-0.038	-0.081	-0.064	-0.064
		(-1.33)	(-0.75)	(-1.33)	(0.199)	(0.197)
-	United Kingdom	-0.116*	-0.041	-0.116*	-0.071	-0.071
		(-1.90)	(-0.79)	(-1.90)	(0.145)	(0.130)
-	United States	-0.107*	-0.040	-0.107*	-0.064	-0.064
		(1.83)	(-0.79)	(-1.83)	(0.186)	(0.176)

Notes: This table contains the ATT using the baseline B&P-filter specification. All estimates are on the common support. For the bootstrap we use 1000 replications. *** p<0.01, ** p<0.05, * p<0.1

[1] Value for maximum distance of controls

[2] imposes common support by dropping # percentage of the treatment observations at which the propensity score density of the control observations is the lowest

Table 7: ATT estimates using different matching algorithms excluding individual countries (five year average)

Variables	Country excluded	(1) Nearest Neighbour without replacement (t-stat)	(2) 5-Nearest Neighbour with replacement (t-stat)	(3) Nearest Neighbour with replacement & calliper=0.01[1] (t-stat)	(4) Kernel bootstrap se (p-value)	(5) Kernel with trimming=3 bootstrap se[2] (p-value)
Total HC expenditures as % of GPD - 5 years average	Australia	-0.067*	-0.064**	-0.067*	-0.070**	-0.075**
		(-1.78)	(-1.98)	(-1.78)	(0.027)	(0.013)
-	Austria	-0.081**	-0.076***	-0.081**	-0.077***	-0.081***
		(-2.25)	(-2.46)	(-2.25)	(0.010)	(0.005)
-	Canada	-0.073**	-0.073***	-0.073**	-0.074**	-0.079***
		(2.01)	(-2.35)	(-2.01)	(0.013)	(0.010)
-	Denmark	-0.073**	-0.064**	-0.073**	-0.065**	-0.070**
		(-2.12)	(-2.17)	(-2.12)	(0.032)	(0.015)
-	Finland	-0.076**	-0.066**	-0.076**	-0.070**	-0.075**
		(-2.06)	(-2.05)	(-2.06)	(0.025)	(0.013)
-	France	-0.080**	-0.071**	-0.080**	-0.074**	-0.079***
		(-2.13)	(-2.29)	(-2.13)	(0.014)	(0.008)
-	Germany	-0.071*	-0.070**	-0.071*	-0.074**	-0.078**
		(-1.86)	(-2.15)	(-1.86)	(0.024)	(0.012)
-	Greece	-0.070*	-0.060*	-0.070*	-0.067**	-0.073**
		(-1.67)	(-1.70)	(-1.67)	(0.050)	(0.039)
-	Iceland	-0.073**	-0.072**	-0.073**	-0.074**	-0.074**
		(-2.01)	(-2.32)	(-2.01)	(0.016)	(0.018)
-	Ireland	-0050	-0.058*	-0.050	-0.072**	-0.077**
		(-1.18)	(-1.76)	(-1.18)	(0.023)	(0.014)
-	Italy	-0.089***	-0.089***	-0.089***	-0.093***	-0.097***
		(-2.56)	(-3.05)	(-2.56)	(0.001)	(0.001)
-	Japan	-0.066*	-0.088***	-0.057	-0.072**	-0.072**
		(-1.76)	(-2.92)	(-1.46)	(0.026)	(0.027)
-	Netherlands	-0.073**	-0.094***	-0.073**	-0.074**	-0.074**
		(-2.01)	(-3.18)	(-2.01)	(0.011)	(0.015)
-	New Zealand	-0.073**	-0.091***	-0.073**	-0.074**	-0.079***
		(-2.01)	(-2.35)	(-2.01)	(0.011)	(0.008)
-	Norway	-0.120***	-0.084***	-0.118***	-0.081***	-0.081***
		(-3.59)	(-2.70)	(-3.26)	(0.008)	(0.010)
-	Portugal	-0.069*	-0.081***	-0.069*	-0.081**	-0.086*
		(-1.87)	(-2.51)	(-1.87)	(0.011)	(0.005)

-	Spain	-0.083**	-0.094***	-0.083**	-0.087***	-0.091*
		(-2.24)	(-2.89)	(-2.24)	(0.006)	(0.004)
-	Sweden	-0.066*	-0.063*	-0.066*	-0.073**	-0.077**
		(-1.72)	(-1.92)	(-1.72)	(0.022)	(0.018)
-	Switzerland	-0.033	-0.070**	-0.033	-0.072**	-0.072**
		(-0.94)	(-2.24)	(-0.94)	(0.015)	(0.016)
-	United Kingdom	-0.075**	-0.068**	-0.075**	-0.067**	-0.067**
		(-1.97)	(-2.12)	(-1.97)	(0.026)	(0.025)
-	United States	-0.073**	-0.068**	-0.073**	-0.074**	-0.079***
		(-2.01)	(-2.17)	(-2.01)	(0.012)	(0.009)

Notes: This table contains the ATT using the baseline B&P-filter specification. All estimates are on the common support. For the bootstrap we use 1000 replications. *** p<0.01, ** p<0.05, * p<0.1
[1] Value for maximum distance of controls
[2] imposes common support by dropping # percentage of the treatment observations at which the propensity score density of the control observations is the lowest

6. Conclusion

The conclusion is that HCF privatisations lead to cost savings in total HCE in the countries included. This finding is robust to various sensitivity tests. Only the alternative B&P-filter specification changes the statistical significance of the results markedly. B&P (1998) show that when the BIC is used for model selection it underestimates the number of breaks. Our results support this finding, the results are less convincing when the BIC is used. Still, the results are in line with the main results, albeit less convincing.

The results suggest an annual average cost saving over the 5-year period of 0.75 percentage points of GDP per year. Accumulated this means that 3.75 percentage points of GDP are saved over 5 years. Thus the analysed policy seems a viable approach to contain or reduce total healthcare costs.

It seems that gradual shifts from public to private financing can lead to lower health care consumption in aggregate. However, we cannot draw conclusions about efficiency. The cost saving might come from less desirable effects of the privatisations. Certain groups are more likely to start under-consuming healthcare. In particular low-income groups are more likely to be excluded due to their budget constraints. Given data restrictions we cannot assess whether the analysed privatisations are causing health-inequality to increase. Furthermore, in a private market for healthcare private payment may cause consumers to choose options of lower quality. Again, we cannot assess whether the analysed reforms are impacting the overall level of health (care) quality.

In conclusion, the results must not be interpreted as if healthcare-financing privatisations will deliver positive outcomes in ‘general equilibrium’. Overall health quality may deteriorate due to a lower quality choice by consumers, and/or because parts of the population are indirectly excluded from the system. Thus, it is not established whether healthcare-financing privatisations are optimal from a positive perspective. If the policy is being pursued with special interest politics in mind, it may be exactly the objective to exclude certain parts of the population, or at least restrict their access compared to high-income groups (Cavaliere & Scabrosetti 2008). Clearly these issues are avenues for future research.

Appendix

Table A1: Identified and validated privatisations

Country	Sample period	1. Global L breaks vs. none, B&P	1. Validated	2. BIC	2. Validated	3. Sequential L+1 breaks vs. L, B&P	3. Validated
Australia	1971-2011	1977	1977	--	--	--	--
Austria	1960-2012	1967	1967	1967, 1989	1967, 1989	--	--
Canada	1970-2012	1986, 1993, 1999	1986, 1993	1986, 1994	1986	1993	1993
Denmark	1971-2012	1984, 1990	1984, 1990	1984, 1990	1984, 1990	--	--
Finland	1960-2013	1994	1994	1993	1993	--	--
France	1990-2012	2003	2003	2003, 2008	2003, 2008	--	--
Germany	1970-2013	1983, 1998, 2004	1983, 1998, 2004	1983, 1998, 2004	1983, 1998, 2004	--	--
Greece	1987-2011	1994	1994	1994	1994	1994	1994
Iceland	1960-2013	1993	1993	1993	1993	1993	1993
Ireland	1960-2012	1985, 2006	2006	1985		--	--
Italy	1988-2013	1994	1994	1994	1994	--	--
Japan	1960-2012	--	--	--	--	--	--
Netherlands	1972-2002	--	--	1996	1996	1998	1998 can be validated
New Zealand	1970-2011	1990	1990	1990	1990	--	--
Norway	1960-2013	1980, 1988, 1997	1988, 1997	1980, 1989	1989	--	--
Portugal	1970-2011	1982, 2006	1982, 2006	1982	1982	--	--
Spain	1960-2012	1995	1995	1989, 1995	1989, 1995	--	--
Sweden	1970-2012	1985, 1992, 2001	1985, 1992, 2001	1985, 1992, 2001	1985, 1992, 2001	2000	2000
Switzerland	1985-2012	--	--	--	--	--	--
UK	1960-2012	1985, 1997	1985, 1997	1985, 1997	1985, 1997	--	--
USA	1960-2012	--	--	--	--	--	--
In total		29	26	28	25	5	5

The table shows the potential privatisations identified using the 3 general test procedures that can be used when applying the B&P-filter. It also shows which of the potential privatisations that can be validated. -- means that the filter does not identify any potential privatisation.

Table A2: Description of data used to identify privatisations

Column 1 and 2 gives the country and sample length. Column 3 shows country specific B&P-filter specification based on the times series properties of each series. Underlined years in column 4 and 5 are the detected privatisations that cannot be validated by the qualitative evidence, the remaining can. The detected privatisation is the first year of the new regime. These are also summarised in the tables above. If a reasonable policy change occurred no more than two years prior to the detected privatisation it is taken as de jure evidence of a reform, see the last three columns.

Country	**Time period**	**Specification of filter based on sample properties**	**Detected privatisation**		**Policy change: Privatisations**		
			BIC	**B&P, global L breaks vs. none**	**Qualitative evidence of *de jure* reforms that privatised (parts) of HCF**	**BIC priv.**	**B&P priv.**
Australia	1971-2011	AR(1) fixed lag specification. Trimming 0.15.	No privatisation identified	1977	• Introduction of universal health insurance (Health Legislation Amendment Act 1975-1983). The public share of total health expenditure jumped in 1975 with the introduction of Medibank, but declined in the late 1970s as a result of the dismantling of Medibank. (Healy et al. 2006)		late 1970’s
Austria	1960-2012	AR(1) fixed lag specification. Trimming 0.15.	1967, 1989	1967	• Act on Health insurance for Farmers of 1965 • Act on Health Insurance for the self-employed of 1966 • Civil Servants’ Health and Work Accident Insurance Act of 1967. • Employment and Social Security Tribunal Act of 1987. • Cost containment transparency: Direct cost sharing for inpatient stay. Reform of 1989. (Hofmarcher & Rack 2006)	1965,66, 67 1987, 89	1965,66, 67
Canada	1970-2012	AR(2) fixed lag specification. Trimming 0.15.	1986, <u>1994</u>	1986, 1993, <u>1999</u>	• The Canadian Health Act of 1984 denies federal support to provinces that allow extra-billing within their insurance schemes and effectively forbids private or opted-out practitioners from billing beyond provincially mandated fee schedules. • Federal transfers frozen in 1990/1991. (Marchildon 2005)	1984 1990-91	1984 1990-91
Denmark	1971-2012	AR(1) fixed lag specification. Trimming 0.15.	1984, 1990	1984, 1990	• Introduction of global budgeting in the publicly financed health sector in 1982. • The first coherent national prevention program for health is developed in cooperation with relevant sectors in 1989. • Budget agreements between the state and the counties increasingly include specific objectives and demands introduced in 1990. (Olejaz et al. 2012)	1982 1989, 90	1982 1989, 90
Finland	1960-2013	AR(1) fixed lag specification. Trimming 0.15.	1993	1994	<u>Milestones in the history of the Finish health care system:</u> • The 90’s: Increasing deregulation and emphasis on municipal autonomy. Reforms in the state administration of health care, subsidy reform. Maintaining health care services during and after economic recession.	1990’s	1990’s

Country	Time period	Specification of filter based on sample properties	Detected privatisation		Policy change: Privatisations		
			BIC	B&P, global L breaks vs. none	Qualitative evidence of *de jure* reforms that privatised (parts) of HCF	BIC priv.	B&P priv.
					(Vuorenkoski et al. 2008)		
France	1990-2012	AR(1) fixed lag specification. Trimming 0.20.	2003, 2008	2003	• Act no. 2002-322 of March 2002. A contractual convention reforming the agreement system between statutory health insurance and healthcare professionals • The 2003 Social Security Finance act: Reference prices for drugs groups, a new system for payment to hospitals, budgets for investment in hospitals among other things. • Drug delisting and reduced reimbursement of pharmaceuticals to reduce costs and increase efficiency • Introduction of flat co-payments to reduce statutory health expenditure in 2005. In 2006 a list of drugs was no longer covered by statutory health insurance. • The 2008 Social Security Finance Act introduced the use of economics in health technology assessments. (Chevreul et al. 2010)	2002-03 2006 208	2002-03
Germany	1970-2013	AR(1) fixed lag specification. Trimming 0.15.	1983, 1998, 2004	1983, 1998, 2004	• 1981 Health Insurance Cost-containment Amendment Act • Health Insurance Contribution Rate Exoneration Act of 1996. Represented a shift from cost-containment to an expansion of private payments. Co-payments were viewed as way to put new money into the system. Further strengthened with First and Second Statutory Health Insurance Restructuring Acts of 1997. • Three months after the government was re-elected in September 2002, it introduced two reform bills with ad hoc austerity measures to reduce expenditure. The 12th SGB V Amendment Act froze ambulatory and hospital care budgets for 2003. (Busse & Reisberg 2004)	1981 1996,97 2002,03	1981 1996,97 2002,03
Greece	1987-2011	AR(1) fixed lag specification. Trimming 0.20.	1994	1994	• Law 2071 of 1992: modernization and organization of the health system. The aim was to replace state responsibility with social security and the private sector in the delivery and financing of health services. Incentives to contract with private insurance were given. Co-payment rates for drugs, per diem hospital reimbursement and insurance contributions were increased. Furthermore, fees were introduced for visits to outpatient hospital departments as well as for inpatient admissions. Tax deductions for private insurance premiums were also adopted. (Economou 2010)	1992	1992

Country	Time period	Specification of filter based on sample properties	Detected privatisation		Policy change: Privatisations		
			BIC	B&P, global L breaks vs. none	Qualitative evidence of *de jure* reforms that privatised (parts) of HCF	BIC priv.	B&P priv.
Iceland	1960-2013	AR(1) fixed lag specification. Trimming 0.15.	1993	1993	• The 1990 Health Care Act. Introduction of out-of-pocket user fees. From 1991 this led to increasing out-of-pocket payments for users of the healthcare system. (Halldorsson 2004)	1991	1991
Ireland	1960-2012	AR(1) No time trend Trimming 0.15.	<u>1985</u>	<u>1985</u>, 2006	• In 2005 the interim Health Information and Quality Authority was established to increase cost effectiveness. Several additional initiatives was introduced, see McDaid et al. (2009).		2005
Italy	1988-2013	AR(1) fixed lag specification. Trimming 0.15	1994	1994	• 1992–1993 The government approved the first reform of the national health system (Legislative Decrees 502/1992 and 517/1993). This involved the start of a process of decentralizing health care powers to the regions and a parallel delegation of managerial autonomy to hospitals and local health units. The latter was envisaged within a broader model of internal market reform. During 1992–1993, co-payments were raised. (Lo Scalzo et al. 2009)	1992 1993	1992 1993
Japan	1960-2012	AR(2) fixed lag specification. Trimming 0.20.	No privatisation identified	No privatisation identified			
Netherlands	1972-2002	AR(1) fixed lag specification. Trimming 0.20.	1996		• 1994 Van Otterloo Act: low-income pensioners became eligible for sickness funds, however other medium income pensioners lost this right. They now had to rely on private insurance. • 1997. The threshold limit for access to sickness funds for pensioners was significantly raised. At the same time students could no longer be insured jointly under parent insurance. A system of limited user charges for sickness fund enrolees was introduced to give them an incentive to use health services more prudently. (Exter et al. 2004)	1994	
New Zealand	1970-2011	AR(1) fixed lag specification. Trimming 0.15	1990	1990	• A Public Finance Act 1989 that made sweeping changes to financial management in the public sector. Chief executives were made responsible for financial management; comprehensive new reporting requirements including statements of service performance; and more emphasis on performance indicators were introduced (French et al. 2001).	1989	1989

Country	Time period	Specification of filter based on sample properties	Detected privatisation		Policy change: Privatisations		
			BIC	B&P, global L breaks vs. none	Qualitative evidence of *de jure* reforms that privatised (parts) of HCF	BIC priv.	B&P priv.
Norway	1960-2013	AR(1) fixed lag specification. Trimming 0.15	1980, 1989	1980, 1988, 1997	• As a result of the Municipalities' Health Care Act of 1982 (1984), responsibility for the primary health care in Norway was transferred to the municipalities in 1984. The government wanted with this act to coordinate the health and social services at the local level, strengthen these services in relation to institutional care, improve resource utilization, strengthen preventive care, and lay the foundation for better allocation of health care personnel. In 1987, the act was extended to include environmentally oriented health activities. In 1988 the Municipalities Health Care Act was further expanded when the responsibility of the counties' nursing homes was transferred to the municipalities. • 1997 activity based financing gave economic incentives to increase patient flow. (Johnsen 2006).	1987-88	1987-88 1997
Portugal	1970-2011	AR(1) fixed lag specification. Trimming 0.15	1982	1982, 2006	• Since 1982 voluntary private health care insurance could be taken out at an individual basis. Before this was only possible at the group level. • Several initiatives from 2003-2006 to reduce public spending: update/increase co-payments, implement purchaser-provider split, pay by results. (HSiT 1999, Baros & Simoes 2007).	1982	1982 2003-06
Spain	1960-2012	AR(1) fixed lag specification. Trimming 0.15	1989, 1995	1995	• In 1986 the General Health Care Act was approved. The process of devolving central public health powers to the regions was completed. • In 1987 health care powers were devolved to the Autonomous Communities of the Basque Country and Valencia. • In 1993 a selective list of pharmaceuticals was excluded from public funding for the first time. Free choice of GPs and paediatricians was generally introduced (piloted since 1984). • In 1994 an agreement was reached amongst the central government and the special Autonomous Communities on the regional resource allocation system, which involved the rationalisation of a set of previous piecemeal, bilateral agreements, and the commitment to renegotiate the terms of the agreement once every four years. (HSiT 2000)	1986-87 1993 1994	1993 1994
Sweden	1970-2012	AR(1) fixed lag specification. Trimming 0.15	1985, 1992, 2001	1985, 1992, 2001	• The 1982 Health and Medical Services Act. Cost containment was an important part of the reform.	1982-85 1992 1998-99	1982-85 1992 1998

Country	Time period	Specification of filter based on sample properties	Detected privatisation		Policy change: Privatisations		
			BIC	B&P, global L breaks vs. none	Qualitative evidence of *de jure* reforms that privatised (parts) of HCF	BIC priv.	B&P priv.
					• The 1985 Dagmar reform continued the decentralization objective of the 1982 reform. The main motive of the reform was to establish county council control over new private establishments through agreements and control over reimbursements to private providers. • The ÄDEL reform of 1992 was the biggest structural reform of health care provision and financing in the 1990's. It contained several initiatives to contain public health care costs. • 1998 Patients' share of the drug costs was increased, as a result of a reformed National Drug Benefit Scheme. In 1999 dental reform that meant an increase in patients' co-payments. (Glenngård et al. 2005)		1999
Switzerland	1985-2012	AR(1) fixed lag specification. Trimming 0.20	No privatisation identified	No privatisation identified	• The health system has only been reformed in 1994 in the data period. The health insurance law made the purchasing of health insurance compulsory and made significant changes to the systems of subsidies within the system. (HSiT 2000)		
United Kingdom	1960-2012	AR(2) fixed lag specification. Trimming 0.15	1985, 1997	1985, 1997	• During the 1980 the Conservative Government introduced a series of initiatives aimed at improving NHS efficiency. • In 1985, a Selected List Scheme was introduced restricting the range of medicines that are available through NHS prescriptions. • In 1997 a new government came into place. It started a whole reform program, massive in scope,that changed the NHS fundamentally. It relied on six principles such as increased de-centralisation and decreased bureaucracy. (Boyle 2011)	1980's 1985 1997	1980's 1985 1997
United States	1960-2012	AR(3) fixed lag specification. Trimming 0.15	No privatisation identified	No privatisation identified			

Data source for detected privatisations: OECD.org, Economic Outlook nr. 90. West German data is used prior 1990 for Germany.
Data source for validated privatisations: WHO/ European Observatory on Health Systems and Policies country HSiT (Healthcare Systems in Transition) reports, see table for precise references.

Table A3: Descriptive statistics of matching covariates

Variables	Obs.	Mean	St.d.	Min.	Max.	Source
GDP growth rate	565	2.718	2.153	-6.371	10.917	OECD.org
Unemployment rate	565	6.490	3.428	0.426	19.108	OECD.org
Interest rate in long-term government debt	565	8.883	5.747	1.003	48.8	OECD.org
Inflation rate	565	5.671	7.712	-0.892	83.95	OECD.org
Government ideology	565	2.853	0.925	1	4	Potrafke (2009)
Government fragmentation	565	0.250	0.259	0	0.806	Beck et al. (2001)
Population share over 65 years	565	13.880	2.571	8.4	20.8	OECD.org
5 year average change in total healthcare expenditures as % of GDP	565	0.114	0.134	-0.255	0.536	Calculated from OECD.org

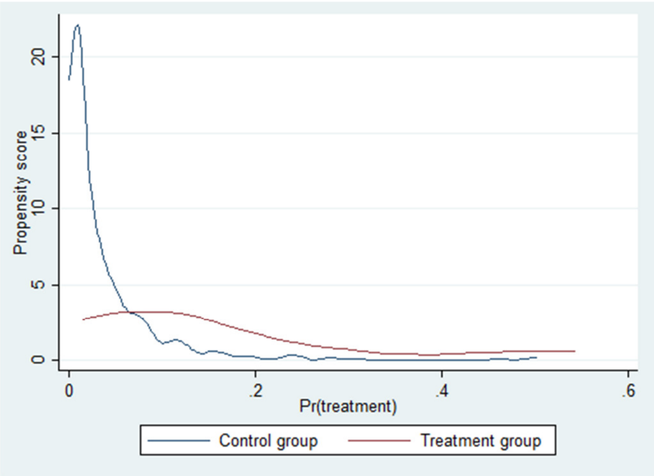

Fig. A1: Common support distribution of the propensity scores for BIC filter specification

Fig. A1 shows the estimated propensity scores for the treated observations and the control observations for the privatisations identified using the BIC filter specification. Concerning treatment observations with a propensity score above 0.5 there is no overlap in the propensity score distributions, i.e. there are no suitable counterfactual(s). For the remaining treatment observations there are suitable counterfactuals, which have a similar propensity of receiving a treatment based on observable characteristics. The privatisations in Denmark 1984, Finland 1993 and Sweden 1992 are dropped due to lack of common support.

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